the *Cyanidæ* commences with an excellent conspectus of the genera of that family, followed by detailed descriptions of both genera and species; two well-executed uncoloured plates accompany the paper.

In the third memoir Dr. T. Thorell gives an account of the *Araneæ* collected in Colorado in 1875 by Dr. Packard; the descriptions of the species are drawn up in the author's usual careful and exhaustive style, and leave nothing to be desired but illustrations, the absence of which we cannot but deplore; an appendix by Mr. J. H. Emerton (the well-known American arachnologist) describes two additional species of the genera *Epeira* and *Drassus*, with which two woodcuts are given.

A. G. BUTLER

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Temperature of Moon's Surface

IN a recent number of *Les Mondes* (tome xliv., No. 1, September 6, 1877), M. l'Abbé F. Raillard puts forward a theory to explain the reddish tinge acquired by the moon during a total eclipse, attributing it possibly to the great elevation of temperature caused by the continuous exposure of its surface for many days previous to the solar rays, which he thinks may be adequate so to raise its temperature as to render it self-luminous.

In support of his theory he refers to my experiments with the thermopile, and states that I have found the lunar surface to acquire under solar radiation a temperature of more than 500° Centigrade. Now in a paper published in the *Proceedings* of the Royal Society, No. 112 (1869), I estimated the radiation to be equal to that of a lamp-blacked surface 500° Fahrenheit higher in temperature at full moon than at new moon, but on repeating the experiments[1] with more care, 197° Fahrenheit, or 100° Centigrade, was found to be a far more probable value, a large error having crept into the former result. It is, moreover, shown in my last paper that near the middle of the partial eclipse of 1872, Nov. 14, the radiant heat was only about one-half of what it had been two hours before, having kept pace in its diminution with the light. Observations made during the recent eclipse, so far as they go, fully confirm this result, and I much doubt if five per cent. of the heat acquired since new moon is retained till the middle of a total eclipse; heat, too, which we have shown from its low mean refrangibility as compared with that of the direct heat of the sun to have been truly *absorbed* by the lunar surface.

M. l'Abbé Raillard appears to be mistaken in supposing it to be the generally received theory that the red tinge is due to dispersion rather than to simple refraction and preponderant absorption of the more refrangible rays in passing through the earth's atmosphere.

There appears to be, therefore, no ground for supposing that the difference between the "lumière cendrée" of the unilluminated surface of the new moon and its reddish hue during a total eclipse is to be ascribed to a difference of temperature, and I think that we must fall back on the usual explanation.

It may also be expected that independently of any tinge due to unequal absorption by the earth's atmosphere the preponderance of blue and green on the terrestrial surface may not be without influence on the colour of the "earth-light" which gives rise to the "lumière cendrée" and may contribute to an appreciable degree towards forming a contrast between its hue and that acquired by the moon when totally eclipsed.

September 15 ROSSE

Rainfall and Sun-Spots in India

As Prof. Balfour Stewart says the true test of a physical cycle is its repetition, and since he evidently regards the tendency to repetition which he has shown to exist in the rainfall of Madras as a favourable indication of the presence of a physical cycle such as that claimed by Dr. Hunter, I may perhaps be allowed to supplement my former statements regarding the tendency of the winter rainfall in many stations of Upper India to vary in a cycle corresponding inversely with the solar spots, by exhibiting a similar tendency to repetition in the rainfall of Calcutta. The following table represents the winter rainfall of Calcutta from 1833 to 1876.

The rainfall is taken for the months of January, February, March, and April in each year, together with that for December of the preceding year. The November fall is excluded chiefly because experience and an inspection of the register show that it properly belongs to the summer monsoon rainfall, occurring almost entirely in those years in which the summer monsoon rains are either very heavy or prolonged, and in fact being nothing else than the last drop they shed before they take their departure. The real winter rains commence in the Christmas week, so December really includes their actual first appearance. As the summer rains seldom begin before the second week in June, we are well within correct limits in taking the rainfall from December to April inclusive. The following table is arranged after the model of that given by Prof. Stewart in his letter to NATURE (vol. xvi. p. 161):—

Years employed.	Years of Series—Calcutta.											
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	
1833–43	4·34	7·34	3·12	2·97	2·16	1·98	3·19	1·24	5·11	7·49	6·69	A.
1844–54	4·51	9·21	6·30	3·85	1·77	6·75	5·79	7·28	9·50	1·60	9·54	B.
1855–65	5·54	3·91	2·76	1·80	7·26	2·56	1·75	5·51	3·83	3·42	8·58	C.
1866–76	7·46	3·21	5·86	8·41	4·83	6·47	5·08	3·02	8·68	5·45	7·49	D.
Whole period average.	5·46	5·91	4·51	4·25	4·00	4·44	3·95	4·26	6·78	4·49	8·07	

The years of minimum sun-spot occur in the first and second series, and the years of maximum sun-spot in the fifth and sixth series. The series of heaviest average winter rainfall are 9, 10, 11, 1, 2, and those of lightest average rainfall are 5, 6, 7, 8. Taking the mean of the averages of the five series of heaviest rainfall we get 6·14 inches, and taking the mean of the averages of the four series of lightest rainfall the result is 4·16 inches. The same result is exhibited by each cycle individually, thus:—

	Max. group, inches.	Min. group, inches.
Cycle A	6·19	2·14
,, B	6·87	4·31
,, C	5·05	4·27
,, D	6·45	4·85

The evidence of repetition is thus quite as manifest as in Dr. Hunter's case, the only difference being that in the present case the years of minimum sun-spot are those of heaviest, and maximum sun-spot those of lightest, rainfall. In order to render it still more apparent that the cyclical connection with the sun-spots is not the result of accident I will exhibit the difference between the rainfalls in years of absolute minimum and maximum sun-spot:—

Years of minimum sun-spot.	Total of the five months.	Years of maximum sun-spot.	Total of the five months.
	inches.		inches.
1834 1844 1856 1867	18·97	1837 1848 1860 1870	11·32
Average for each year	4·74	Average for each year	2·83

The same connection is maintained when the fall in April is left out or that in November included, so that it is evidently not due to the effect of any particular month, but as may reasonably

[1] *Philosophical Transactions*, 1872.

be inferred, an indication of the presence of some physical cause tending to increase the rainfall in years of minimum and diminish it in years of maximum solar maculation.

Bankipore, Patna E. D. ARCHIBALD

The Australian Monotremes

IT is certainly news to me, and I believe to most other European naturalists, that *Tachyglossus* and *Ornithorhynchus* occur in Northern Queensland. Perhaps W. E. A. will kindly state, for our information, the exact spots where they have been discovered and their extreme *northern* limit, so far as this has been ascertained.

W. E. A. speaks of an adult female *Echidna* (*sive Tachyglossus*) having "a fine healthy young one in the pouch." Is there not some error here, as the monotremes have, strictly speaking, no marsupial pouch? P. L. S.

WITH reference to the existence of *Tachyglossus* (*olim Echidna*) in North Australia, and the recent discovery of one (or possibly two) species in New Guinea, the following account, which I lighted on a few evenings ago, when looking over an old volume of the *Field*, seems to be of sufficient interest to warrant its transfer to the pages of NATURE. The account in question occurs in an article "A Week at Plain Creek, Queensland," by Mr. E. B. Kennedy, which appeared in the issue of that journal for September 20, 1873. It runs as follows:—" . . . Whilst so engaged we heard our dogs making a tremendous noise, high up the bank in the scrub, and upon going to ascertain the cause found them scratching, yelling, and pulling at a porcupine which was half imbedded in a hole; we were at least ten minutes digging him out with sharp-pointed sticks, such was his tenacity in holding on and burrowing. The quills were not nearly so long as the Cape of Good Hope species (of course a true *Hystrix*), and he differed from that quadruped in having a sort of beak instead of a regular jaw." It is to be regretted that Mr. Kennedy did not preserve his specimen, which was ultimately cooked and eaten! I should have mentioned that Plain Creek lies in 21 lat. S., so that this is certainly the northernmost locality on the Australian continent, where we have certain knowledge that the *Echidna* occurs. As we now know that many North Australian species of birds range also into southern New Guinea, it would hardly be surprising if the *Tachyglossus* of the Fly River and south New Guinea were nothing more than the well-known *Tachyglossus hystrix*. It is to be hoped that this point may soon be solved by the arrival of specimens from both localities. W. A. FORBES

English Names of Wild Flowers and Plants

To all who are interested in the history of the English language the derivations proposed for the vernacular names of many plants in the Rev. W. Tuckwell's lecture (see NATURE, vol. xvi. p. 385) will be highly appreciated. And even in the few cases where the etymologist may feel doubtful as to the verisimilitude of the suggested pedigree it will for the most part be difficult to propose another with any great confidence.

There is, however, one of these doubtful cases, the derivation of woodruff from wood-roue, in lieu of which I have to offer a conjecture which appears to need no lengthy argument to insure its acceptance.

Is not the ruff of woodruff identical with the riff of sherriff? Is not, in short, the woodruff the wood-reeve, just as the sherriff is the shire-reeve? That the German wald-meister has the same connotation and is applied to the same plant is evidently a striking confirmation of this view, and it would be interesting to know whether the word wald-graf (*i.e.*, wald-ge-raf = wood-y-reeve), or any equivalent form, is to be met with in high or low German literature.

I used to be told by a school-fellow that the way to spell woodruff was—

Double U, double O, double D, E,
Double R, double O, double F, E.

Even under the disguise of woodderrooffe, however, the origin of the word is perceptible.

As regards the main purpose of the Rev. W. Tuckwell's paper, I feel strongly that scientific accuracy is compatible with a much freer use of vernacular words than is customary amongst us, and that their adoption by science teachers would remove a great stumbling-block from the path of learners.

Manningham, September 10 J. WILLIS

Some of the Troubles of John O'Toole respecting Potential Energy

"IT is the people's *right* to demand of their teachers that the information given them shall be, at least, definite and accurate as far as it goes," and "whenever there appears to be a confusion about fundamental principles it is the *duty* of a scientific man to endeavour by all means in his power to remove it." These are the words of one of the teachers.[1] I am one of the people—as indeed, my name testifies, Toole (*Tuathal*) being the Irish equivalent of the Latin *Publius*—and I would now, on behalf of myself and every brother Publius, assert our above "right" in respect of the matter now in hand, and demand the performance by the doctors of their corresponding "duty." Now there is much "confusion about the fundamental principles" of physical Energy in the minds of the public who care about such things; and it is principally, though I admit not entirely,[2] the doctors who are to blame for this. Their own ideas on the subject being so clear and correct they are superior to the phraseology they use respecting it, and they are not injuriously affected thereby; but those who are dependent on that phraseology for their knowledge are in very different case. Let me, as one of the latter, point out some of the perplexities under which we labour from no fault of our own, and which we should be spared if our teachers would only condescend to use their words discreetly and consistently. It may be well to premise that we know the definition of physical Energy, which is—"the power or capacity of performing work;" and that we are not now making any confusion between Energy and force.

The word "potential" has two very different meanings—(1) Of, or belonging to, potency or power; (2) Existing *in posse*, or in possibility, as opposed to existing *in esse*, or in actuality; and the expression, "potential Energy," can have no less than three references or meanings, which we shall mark with A, B, and C; and each meaning has its own proper inconveniences independent of the perplexities arising from their mutual relations.

A.—*Potential E., as meaning "Energy existing* in posse."

The phrase "potential E." is in the first place very generally intended to mean E. existing *in posse*, according to one proper signification of the word "potential." The phrase was first used by Rankine,[3] and apparently in this sense; he contrasted "potential" and "actual" E. This antithesis is still very generally implied and sometimes expressed. Clerk Maxwell tells us[4] (the statement being *repeated* only last year[5]) that "potential E." "signifies the E. which a system has not in actual possession, but only has the power to acquire." Wormell says[6]—"It has been aptly called possible or *potential* E., because it represents the power the body has of acquiring actual or kinetic E." Many of our doctors use the phrase "potential E." without explaining it, and of course, unless there be some particular reason to the contrary, such must be understood to give it, as one of its significations, at least, the original meaning intended by its proposer (or if not they are guilty of a very misleading omission, *utrum horum mavis accipe*); and this is especially undeniable in the case of those who apply the title "actual" to the other type of E. Balfour Stewart, though he seems to have quietly dropped the name "potential,"[7] has really retained the idea implied thereby, for he still habitually calls the other type of E., that of motion, "actual E.," as its *distinguishing* title. Moreover, this idea is involved in other statements, &c., of our teachers. For instance, we occasionally find language used which seems to imply that potential E. must first emerge as actual E. before it can produce work, as by Deschanel,[8] by Dunbar Heath,[9] and by Balfour Stewart.[10] Observe, also, the expression "E. of actual motion," which is frequently used by the last-mentioned doctor,[11] and accepted at least by Tait.[12] "E.

1 Tait Evening lecture during meeting of Brit. Assoc. at Glasgow in 1876. NATURE, Sept. 21, 1876.

2 That brother Publius who wrote the article on Tyndall's "Heat," in *Blackwood's Mag.*, December, 1863, was partly responsible for his own confusion about Energy.

3 *Phil. Mag.*, February, 1853. He says: "All conceivable forms of E. may be distinguished into two kinds, actual or sensible, and potential or latent."

4 "Theory of Heat," p. 91, 1871.

5 "Matter and Motion," p. 81, 1876.

6 "Dynamics," p. 185.

7 At least it never occurs in his "Conserv. of Energy," 1874, though frequently in "Elem. Physics," 1870.

8 "Nat. Phil.," p. 78, edition of 1870.

9 "Energy," p. 64. 10 "Elem. Physics," pp. 104–106. But see p. 360.

11 "Conserv. of Energy," p. 25, and elesewhere.

12 "Unseen Univ." (last edition), p. 109, twice.